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J. B. Rhine's *Extra-Sensory Perception* and Its Background in Psychical Research

By Michael McVaugh* and Seymour H. Mauskopf**

I

THE ORGANIZED INVESTIGATION of psychical phenomena, inaugurated with the establishment of the British Society for Psychical Research, is now ninety-four years old. For slightly less than half this period—since the early 1930s—there has been a continuing attempt to make these investigations into an experimental science, an attempt which originated in the work of J. B. Rhine and his associates at Duke University. We mean in this paper to consider the significance of Rhine's early work for the "internal" history of parapsychology, as the field has come to be called.¹ In what ways did this work, summarized in Rhine's first book, *Extra-Sensory Perception* (published in 1934), give focus and definition to earlier attempts to devise an experimental basis for psychical research? Put in more familiar terms, in what manner can Rhine's book be said to have furnished a paradigm for experimental work in this field? There have been many casual accounts of the early years of experimental parapsychology or psychical research, and most of them credit Rhine's early work with having transformed the field.² But these histories tend to be narrative rather than analytical and are generally sketchy about the relationship of Rhine's work to earlier experimental attempts as well as the impact of Rhine's book of 1934 upon the course of subsequent parapsychological research. In order to explicate the paradigmatic nature of Rhine's work,

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¹The term originated in the late nineteenth century in Germany and by the 1920s was in general use there. The journal *Psychische Studien*, e.g., changed its title to the more imposing *Zeitschrift für Parapsychologie* in January 1926.

²One particularly succinct statement—by an English researcher in the field—declared: "The modern phase of experimental work . . . may be regarded as beginning with the publication in March [actually April] 1934 of Dr. J. B. Rhine's book, *Extra-Sensory Perception*." Whately Carington, *Telepathy* (London: Methuen, 1945), p. 20. Discussions which seem to us in essential agreement with this attitude (although they do not put the conclusion quite so forcefully) may be found, e.g., in Gardner Murphy, *Challenge of Psychical Research* (New York: Harper, 1970), pp. 65 ff., or Robert H. Thouless, *From Anecdote to Experiment in Psychical Research* (London: Routledge and Kegan Paul, 1972), pp. 33 ff.



J. B. Rhine (right) testing Hubert Pearce by the "D.T." (down through) method. Pearce is calling uninterruptedly down through a pack of twenty-five Zener cards.

we will eventually need to deal with both these issues, but we shall devote most of our attention to the former.

Before proceeding, however, we should call attention to certain features of parapsychology which differentiate it from more orthodox sciences, even though they do not figure prominently in our paper. For one thing, it would be unrealistic to assert that parapsychology has found general acceptance in the scientific community; rather, the field continues to arouse considerable controversy and even hostility. It should also be recognized that parapsychology has never entirely resolved its problems with evaluating experimental methods and data, problems which turn principally upon the difficulties of replicating apparently positive results and even upon the issue of what constitutes "positive results." These problems have certainly affected the way Rhine's work was received by the scientific community and even, on occasion, by other parapsychologists; nevertheless, we feel it is beyond the scope of the present paper to take them up in any detail. Our concern will be simply to show where Rhine's book stood in relation to earlier attempts at experimental research—which Rhine himself accepted essentially at face value in 1930–1934—and to understand how psychical researchers reacted to the book when it first appeared. We do not feel it necessary to pass scientific or philosophical judgment upon Rhine's work or to argue for or against the reality of extra-sensory perception.

It is first of all important to be clear as to the contents and aims of *Extra-Sensory Perception*. That it was indebted to an existing tradition of psychical research is suggested by the fact that it first appeared in April 1934 as a publication of the Boston Society for Psychic Research, although in 1935 it was reprinted by the publisher Bruce Humphries.³ But a stronger and more revealing expression of that debt is Rhine's account therein of the historical background to his three years' research, which makes up the first section of the book. The account makes clear the strengths and weaknesses in Rhine's knowledge of the field, to which he had come fairly recently from botany. He was quite well acquainted with English and American contributions to psychical research (though some he knew of only at second hand), but far less so with continental work.⁴ This is perhaps fair enough, since only Rudolf Tischner in Germany (whom Rhine had read) and René Warcollier in France (whom he had not) were then carrying on anything approaching systematic experimentation on mental phenomena. It should be remarked, however, that once Rhine's own experiments gained impetus in late 1930 he began to be rather casual in his reading of even the English material; for example, his historical account mentions only in passing the experimental reports published by S. G. Soal in the *Proceedings of the Society for Psychical Research* in 1931 and 1932. These limitations on Rhine's knowledge of other psychical research are of some importance as having established a set of expectations for his own work, as we shall see.

³Our references are to the latter printing: J. B. Rhine, *Extra-Sensory Perception* (Boston: Bruce Humphries, 1935), which will be referred to in subsequent footnotes as *ESP*. The two printings are, however, identical. The text of the recent reprint of *Extra-Sensory Perception* (Boston: Bruce Humphries, 1964) has been altered in a number of significant respects and is entirely unsatisfactory as a source for Rhine's thought in the 1930s.

⁴Rhine had abstracted a few foreign articles from such journals as *Psychische Studien* and the *Revue Métapsychique* for the *Journal of the American Society of Psychical Research* in 1925 and 1926 but thereafter saw nothing of these journals. He knew of early continental work through Charles Richet's *Thirty Years of Psychical Research* (1923), which he read in 1926.

That work was described in detail in the succeeding section, "The Experimental Results," which explained the techniques introduced by Rhine and his colleagues to permit statistical study of an isolable instance of extra-sensory perception and then described the actual course of research carried on with half a dozen Duke undergraduate and graduate students who, in preliminary testing from 1930 on, had seemed potentially good subjects. The actual technique employed was beautifully simple. Students were asked to guess cards from a specially designed deck of twenty-five, containing five each of five suits (crosses, wavy lines, stars, rectangles, and circles). Chance expectation would be one correct guess in five, five in a run of twenty-five, and success of greater than 20 percent was evaluated statistically for its rarity. Rhine's principal percipient subjects—A. J. Linzmayer, Charles Stuart, and Hubert Pearce—could score consistently from six to ten successes out of twenty-five trials, guessing cards in any of a number of ways: when the experimenter-agent, hidden from the percipient, was looking at each card (G.E.S.P., general extra-sensory perception); when the agent was holding or touching the card being called but did not know what suit it was (P.C., pure clairvoyance); or when the agent simply shuffled the cards and left the deck as a whole, hidden from the percipient, for the percipient to guess in order going down through the deck (D.T., down through; see the Frontispiece). Pearce, for example, had called 3,049 hits in 8,075 trials (averaging 9.4 per 25) by pure clairvoyance in one series, a record whose likelihood Rhine calculated as something over 10^{-320} !⁵ *Extra-Sensory Perception* also recorded some attempts to study these phenomena under widely varying conditions: when the subject had been given sedatives or stimulants, or when agent and percipient were separated by considerable distances—of 250 yards and even 250 miles. Again, extra-chance success was repeatedly reported.

The whole series of experiments was discussed and interpreted in the concluding section, "Explanation and Conclusions." It is to this section that one might naturally turn in trying to understand the book's success within parapsychology, expecting some significantly new element of understanding to be presented. But while to the layman Rhine's conclusions and his structuring of the subject matter might have seemed novel, to someone who had carefully studied the work of earlier psychical researchers this would have had a familiar ring. Certainly Rhine himself had no thought of claiming the establishment of a new science. Indeed, he was insistent to underline his dependence upon the accomplishments of such individuals as Charles Richet and Mrs. Henry Sidgwick. *Extra-Sensory Perception* is in fact a synthetic exposition, based upon new research, of previous conclusions going back half a century, and its power over parapsychologists must surely have lain in part in the fact that it incorporated in coherent fashion all the truths of psychical research that two generations of students had laboriously uncovered but had not yet properly drawn together. This judgment is of enough interest to warrant detailed study of Rhine's conclusions, while exploring their relation to those of his predecessors.⁶ At

⁵ *ESP*, pp. 85, 168.

⁶ Because we are arguing that Rhine's work was a synthetic accomplishment, we have taken some pains to offer illustrative material from works that can be clearly identified as having formed part of his intellectual background in 1934. Books or articles cited in the footnotes to which Rhine himself made reference in *ESP* will be indicated with an asterisk when first cited; of course, works not so identified may still have been known to him.

the same time we can make clear the paradigm laid down in the book that served to guide much subsequent parapsychological research in the 1930s.

II

It should first of all be emphasized that even the experimental methodology which Rhine employed had a venerable tradition going back to the early days of organized psychical research. When psychical phenomena first became a subject of amateur scientific study, one topic repeatedly investigated was thought transference, on the hypothesis that this phenomenon gave support to the possible existence of discarnate spirits. In the late 1870s the French physiologist Charles Richet (who was to win the Nobel Prize in 1913 for his work on anaphylaxis) began a series of experimental investigations into such phenomena. More important, the Society for Psychical Research (S.P.R.) tried to encourage some sort of ongoing tradition of investigation from its inception in 1882. Its leaders encouraged members to undertake their own researches to try to demonstrate communication of thoughts, and for more than a decade such researches were widely carried on and regularly reported in the *Proceedings* and *Journal* of the Society. There was no consistent pattern to these early experiments: tastes, colors, commands, pictures, imaginary scenes, playing cards, and numbers were all subjects chosen for experiments. Particularly frequent in the late 1880s were reports of playing-card experiments, in which the percipient tried to identify the suit of the card drawn, or the number of pips on it, or perhaps the very card itself.

These first experimenters were certainly not blind to the fact that simple entities like numbers and cards held a particular advantage, for their successful or partial transmission could be more satisfactorily measured than results with complex forms. Among the earliest of the experimental investigators, Richet in 1884 had worked with cards among other objects and had observed broadly that given a large number of attempted communications, a pronounced variance between expected and achieved successes would indicate that something besides accident was at work.⁷ Richet's discussion immediately led the physicist Oliver Lodge and Edmund Gurney (an officer of the S.P.R.) to try to derive an algorism that would permit an experimenter to judge, for example, what sort of success (guessing suits, colors, cards) is how improbable.⁸ F. Y. Edgeworth, a barrister with an interest in mathematics, did the same in two much more sophisticated papers (perhaps too difficult for their immediate audience) in the *Proceedings* of the S.P.R. for 1885 and 1886.⁹ Gurney, reviewing Richet's work in the *Proceedings*, had urged the interested public to attempt such experiments themselves:

⁷*Charles Richet, "La suggestion mentale et le calcul des probabilités," *Revue Philosophique*, 1884, 18:609-674.

⁸*Edmund Gurney, "M. Richet's Recent Researches in Thought Transference," *Proceedings of the Society for Psychical Research*, 1883-1884, 2:239-264. Lodge's approach is given (in a letter from Lodge and his brother, Alfred, to Gurney, dated Jan. 8, 1885) on pp. 257-262 and Gurney's on pp. 262-264.

⁹*F. Y. Edgeworth, "The Calculus of Probabilities Applied to Psychical Research," *Proc. S.P.R.*, 1885, 3:190-199; "The Calculus of Probabilities Applied to Psychical Research II," *Proc. S.P.R.*, 1886-1887, 4:189-208.

We are often asked by acquaintances what they can do to aid the progress of psychical research. These experiments of M. Richet's suggest, at any rate, one answer; for they can be repeated, and a valuable contribution made to the great aggregate, by any two persons who have a pack of cards and a little perseverance. One person should draw a card at random from a pack, and regard it steadily; the other should try to guess it, and his success or failure should be silently recorded. The pack should be shuffled after each trial, and the number of trials made at any one time should be limited to 50. The total number of trials contemplated (1,000, 5,000, 10,000, or whatever it may be) should be specified beforehand; and in order that the guesser's mind may be in as blank and receptive a state as possible, he should not be allowed to see the record of results until the whole series is completed.¹⁰

Yet, whether because the task seemed too mechanical and boring or not closely enough connected with original, spontaneous thoughts,¹¹ or because the difficulties involved in applying the calculus of probability seemed scarcely worthwhile,¹² reports of card-guessing became quite rare in the 1890s and appeared only infrequently for several decades. Those who reported card-guessing experiments tended at most to compare their results with what would be achieved by chance, without attempting to come to grips with the statistical question of just how improbable their results had been.¹³ Mrs. A. W. Verrall, who carried on several thousand trials under varying circumstances, was the last person who pursued the idea at all seriously; in 1894 she had C. P. Sanger apply Edgeworth's techniques to her results, which seemed to indicate a certain degree of success, especially in guessing suits.¹⁴ Richet himself did not attempt to extend the statistical side of his researches, although he continued to carry on experiments that would permit him to evaluate success as compared to chance. Nevertheless, Richet long insisted upon the strengths of the statistical methods that he had helped to introduce. In *Thirty Years of Psychical Research*,

¹⁰Gurney, "Richet's Recent Researches," p. 242. Cf. *Edmund Gurney, Frederic W. H. Myers, and Frank Podmore, *Phantasms of the Living*, 2 vols. (London: Society for Psychical Research, 1886), Vol. I, pp. 31–35 for a summary of the spate of card-guessing experiments carried out in the previous two years; pp. 24–26 for an account of Edgeworth's statistical analysis of the successes of guessing cards, numbers, and words by the Creery sisters; pp. 71 ff. for an account of Richet's attempts to calculate probabilities of guessing letters in "table-turning" experiments. In 1887 Gurney wrote: "the examination of probabilities, and in cases which admit of numerical measurement the application of the calculus of probabilities, is not only adapted to decide the question of thought-transference, but is the one indispensable means of deciding it." Review of Alfred Binet and Ch. Féré, *La magnétisme animal*, in *Proc. S.P.R.*, 1886–1887, 4:543.

¹¹See Gurney *et al.*, *Phantasms of the Living*, Vol. I, pp. 34–35, for a criticism of card experiments along these lines. One does occasionally find later defenses of the statistical evaluation of card-guessing and the like as offering more satisfactory proof of psychical abilities than spontaneous occurrences. Cf. Alice Johnson, "Coincidences," *Proc. S.P.R.*, 1898–1899, 14:183.

¹²*Malcolm Guthrie, "Further Report on Experiments in Thought-Transference at Liverpool," *Proc. S.P.R.*, 1885, 3:427; Charles Richet, "Relation de diverses expériences sur la transmission mentale, la lucidité, et autres phénomènes non explicables par les données scientifiques actuelles," *Proc. S.P.R.*, 1888–1889, 5:148–152, where Richet reported *no* significant results in his statistical experimental work with the guessing of playing cards.

¹³See, e.g., *Oliver J. Lodge, "Some Recent Thought-Transference Experiments," *Proc. S.P.R.*, 1891–1892, 7:377, where he reports simply that "out of 16 guesses of cards only 6 were wrong"; "out of 39 trials 16 were correct and 23 wrong" (p. 381), without trying to deal with the measurable improbability of such a series of successes. Or Edmund Gurney *et al.*, "Second Report on Thought-Transference," *Proc. S.P.R.*, 1882–1883, 1:76–77: "excluding the 2nd trial the successful results were rather more than 1 in 3; pure chance would have given 1 in 16."

¹⁴*Mrs. A. W. Verrall, "Some Experiments on the Supernormal Acquisition of Knowledge," *Proc. S.P.R.*, 1895, 11:174–193.

published in 1923, he provided a very widely ranging survey of the English and continental researches, in the course of which he described earlier card-guessing experiments and repeated his conviction that “these methods are not emotional or dramatic like experiments made with powerful mediums, or records of monitions of death, but they are precise, and, when the experiment is well designed, undeniable.”¹⁵

It is natural enough that Richet should sound somewhat apologetic about this view. Ever since the 1890s, what experimental work was being reported on thought transference had centered largely upon attempts to convey vivid, complex thoughts that would be laden with meaning and emotion for the agent communicating the thought—presumably to be more readily received. When successful, such experiments seemed vastly impressive. Professor Gilbert Murray, the Oxford classicist, used to carry off such feats as after-dinner amusement. Friends would agree on an event or a scene from literature in his absence; then he would join them, take the hand of one of their number, and with remarkable accuracy identify the imagined scene. The following exchange made up a part of the game of November 23, 1910: “Subject: Mrs. Arnold Toynbee (agent): ‘Out of *l’Espion*. Evsei finding a fly in his ink.’ Professor Murray: ‘This is a book—a Russian book. It’s *l’Espion*. It’s a scene I don’t remember at all. I get an impression of the boy squashing a fly, but I can’t remember it at all. I confuse it with Joseph Vance.’”¹⁶ By this time it was the dramatic, overwhelming proof of thought transference rather than mere statistical evidence of its presence that was being sought by most investigators.¹⁷

Until about 1915, virtually all attempts to study thought transference experimentally were carried out in the context of the psychical research societies, in particular the S.P.R. The investigators were often of great intellectual and social distinction, as names like Henry Sidgwick, Oliver Lodge, Gilbert Murray, and A. J. Balfour will suggest. But the conduct of psychical research remained essentially an amateur affair, in the sense that while these individuals may well have been intensely involved in psychical pursuits, their activity was never a part of their professional careers. This situation continued to hold true in England for another twenty years. Elsewhere, however, some experimental psychical research now began to be carried on sporadically within the universities by professional psychologists. Their work was in general based upon carefully delimited experimental situations that could be given statistical treatment. These

¹⁵*Charles Richet, *Thirty Years of Psychical Research*, trans. Stanley De Brath (New York: MacMillan, 1923), pp. 96, 98; his summary of earlier statistical work covers pp. 85–99. This book was first published in French as *Traité de métapsychique* (Paris: Alcan, 1922).

¹⁶Mrs. A. W. Verrall, “Report on a Series of Experiments in ‘Guessing,’” *Proc. S.P.R.*, 1918, 29:97.

¹⁷Cf. W. H. Salter’s comments concerning the early turn of interest away from the statistical evaluation of card-guessing experiments to concentration on what he calls “free” material, incapable of precise statistical analysis. “An Appeal for Co-operation in Further Experiments in Extra-Sensory Perception,” *Proc. S.P.R.*, 1935, 43:38. This is a particularly significant comment, since the appeal is made apropos of Rhine’s *Extra-Sensory Perception*.

Already in 1896, William James said in his presidential address to the Society for Psychical Research that “experimental thought-transference has yielded a less abundant return than that which in the first year or two seemed not unlikely to come in. Professor Richet’s supposition that if the unexplained thing called thought-transference be ever real, its causes must, to some degree, work in everybody at all times (so that in any long series of card-guessings, for example, there ought always to be some excess of right answers above the chance number) is, I am inclined to think, not very well substantiated.” “Address by the President,” *Proc. S.P.R.*, 1896–1897, 12:3.

men, who often were openly skeptical or hostile toward the possibility of telepathy, were seeking not for complete transmissions of thought but for evidence of even fragmentary instances of telepathy. They did not, however, establish a common experimental technique.

In the United States private endowments to Stanford and Harvard for psychical research made it possible for psychologists to carry out and publish studies in this field. From 1912 to 1917 J. E. Coover at Stanford attempted to repeat some of the original tests tried by members of the Society for Psychical Research—the guessing of lotto-block numbers or of the suit, color, and number of playing cards.¹⁸ In 1917 L. T. Troland of Harvard University asked subjects to predict which of two lights would be illuminated;¹⁹ in 1919 Heymans and Brugmans, at Gröningen, asked a subject to identify a particular checkerboard square being chosen by an agent elsewhere.²⁰ All these situations allowed the researcher to calculate easily the chance likelihood of success and to determine the degree of improbability of the percipient's actual accomplishments—although only Coover and Troland actually tried to calculate the odds against their subjects' successes. In various ways, each of these academic psychologists extended the methodology of the card-guessing situation: Coover, by taking elaborate protocols of his subjects' introspections; Troland, by attempting to mechanize the test situation as completely as possible; Brugmans, by testing his subject under the influence of drugs as well as in a normal state. Coover's experiments, carried out over a five-year period, were by far the most ambitious, but Coover's negative conclusion about the existence of telepathy seems to have kept him from any further research. Troland was content to stop after a mere 605 trials, and Brugmans' tests only involved brief work with a single subject. A commitment to sustained experimentation was certainly not general. When G. H. Estabrooks tried his own experiments at Harvard in the years 1924–1925, knowing of Coover's work, he made card-guessing the format; but he recorded only 1,660 guesses. His statistical analysis consisted in determining the probable error in each series and pointing out that the actual success exceeded the probable error by factors of up to 6.04.²¹

¹⁸*John Edgar Coover, *Experiments in Psychical Research at Leland Stanford Junior University* (Stanford University, Psychical Research Monograph No. 1, 1917).

¹⁹*Leonard Thompson Troland, *A Technique for the Experimental Study of Telepathy and Other Alleged Clairvoyant Processes* (Albany, n.d.).

²⁰*H. J. F. W. Brugmans, "Une communication sur des expériences télépathiques au laboratoire de psychologie à Groningue . . .," *Compte rendu officiel due premier Congrès International des Recherches Psychiques* (Copenhagen, 1922), pp. 396–408; cf. G. Heymans, H. J. F. W. Brugmans, A. A. Weinberg, "Een experimenteel onderzoek betreffende telepathie," *Mededeelingen der studievereeniging voor "Psychical Research,"* 1921, 3–7.

²¹*G. H. Estabrooks, *A Contribution to Experimental Telepathy* (Boston: Boston Society for Psychic Research, Bulletin V, 1927). An additional piece of experimental work, seldom if ever cited and in some ways strikingly prophetic of the experimental methods to be employed by Rhine, was carried out by Professor Hulsey Cason of Syracuse University and published in the *Journal of the Society of Psychical Research* in 1924. As telepathic material Cason employed a deck of forty cards made up of ten cards each of four geometrical symbols: square, circle, triangle, and rectangle. Telepathy and clairvoyance were not differentiated. In Cason's experiments 3,040 trials were made with the agent and percipient, or "re-agent," in different rooms; 2,400 trials were made with them in the same room but with the cards shielded by a screen. Cason did not attempt to calculate the degree of improbability of the aggregate results, since the difference between the average scores in each type of trial (respectively 25.36 and 25.25 correct out of 100) and chance expectation of 25 per 100 were judged qualitatively by Cason as too insignificant to support any suggestion of telepathy.

In the preface to the paper the editor of the *J.S.P.R.*, Mrs. W. H. Salter, defended the Society's

In many ways the most elaborate analysis of experimental results carried on in the decade before 1934 was done not in the academic community but by a member of the S.P.R., Ina Jephson. Between 1924 and 1927 Miss Jephson performed or collected accounts of some six thousand attempts at "card-divination."²² Her experimenters, at her instructions, made five series of five consecutive guesses at a playing card drawn from (and then returned to) an ordinary deck of fifty-two, recording their results in detail. Following a system of scoring devised for her by the statistician R. A. Fisher,²³ Miss Jephson was able to evaluate the varying degrees of success that might be observed in card-guessing: to determine, in effect, how much more improbable it would be to identify the four of hearts as the five of diamonds than as the king of clubs. Miss Jephson was here going under the reasonable assumption (under an analogy with sight) that mechanisms might be at work in card-guessing which could be identified by treating closeness to number or to suit as partially successful. In 1928 she published her results, which included not merely extra-chance success but an apparent decline-of-success curve that fitted almost every subject's guessing.

Miss Jephson's work is of still greater interest because it gave rise to a brief debate that made explicit the mixed feelings of psychical researchers about the value and interest of a technique designed to show merely statistical success. Her initial appeals for card-guessing data had stung a French student of the field, René Sudre, to attack her projected experiment. In early 1928, before her results were published, he begged investigators not to perform collective experiments of this sort.²⁴ His argument was that it was a single definitive experiment that was needed; that the use of statistical reasoning was unconvincing and beyond the competence of most psychical researchers. Miss Jephson replied briefly to his criticisms in her report, but he repeated them the next year in somewhat greater detail, now adding a complaint against her practice of evaluating cases of partial success and insisting that only precise identification of a guessed card as to both suit and number should be scored as success. Vastly more convincing than her reported results, he insisted, would have been the report of a single case in which five consecutive cards were identified on five separate occasions.²⁵

relative dearth of experimentation of this kind in the early 1920s by reiterating what seems to have been the prevalent view in the Society at this time: "So far as can be judged by the circumstances in which thought-transference appears to occur spontaneously, the state of mind induced by time experiments conducted on such rigid lines is not a state of mind conducive to the reception of telepathic impressions" (p. 314). "A Simple Test for Thought-Transference," *J.S.P.R.*, 1923-1924, 21:314-319. See above, n. 11.

²²*Ina Jephson, "Evidence for Clairvoyance in Card-Guessing," *Proc. S.P.R.*, 1928-1929, 38:223-271.

²³*R. A. Fisher, "A Method of Scoring Coincidences in Tests with Playing Cards," *Proc. S.P.R.*, 1924, 34:181-185. For Fisher's revision of this method to account for the effect of card preferences in subjects, see Jephson, "Evidence for Clairvoyance," pp. 269-271.

²⁴In the publication *Psychic Research* for February 1928; reported in Ina Jephson, "A Reply to M. Sudre's Article 'An Experiment in Card Guessing,'" *Proc. S.P.R.*, 1930-1931, 39:185-189, and in "Evidence for Clairvoyance," p. 264.

²⁵The success of Rhine's work caused Sudre to modify his position somewhat by 1956 in his *Traité de parapsychologie*: "The statistical method which Rhine applied to the guessing of playing cards is of a kind to reassure them [scientists], the more so as these long sequences of guesses allow of no dramatic or emotional accompaniments. In this way the American research worker

The issue here has continued to be a subject of debate within psychical research: what *can* the researcher consider as proof of psychical ability? Miss Jephson responded to Sudre's objections by arguing that what constituted *proof* in science was a repeatable effect, in this case a continuing statistical regularity, rather than a unique event.

As an example of an experiment which would be unanimously considered as above chance, he gives a picturesque illustration, imagining the case of a monkey, playing on a typewriter, reproducing his article in *Psychic Research*. But however undeniably above chance this event might be considered, I am afraid that M. Sudre would be disappointed in its effect on scientific opinion. I feel certain that indifference would remain until M. Sudre could exhibit his gifted monkey writing articles regularly for *Psychic Research*. . . . Scientific conviction, after all, is only the name we give to the effect of the prolonged persuasion due to repeatedly observed facts or events.²⁶

Fisher independently explained the importance of "statistical significance" for anyone investigating biological phenomena, pointing out that this criterion prevents the scientist from being deceived by circumstances that he cannot control.²⁷ Where honors lay in the debate is hard to decide. France continued to resist the use of mathematics in psychical research, while England saw a flurry of attempts to perform experiments amenable to statistical treatment, the most important of which were conducted by the mathematician S. G. Soal. He and Theodore Besterman joined Miss Jephson in 1929 in a repetition of her clairvoyance experiments on a much more ambitious scale, only to report with disappointment in the 1931 *Proceedings* that they had been totally unable to verify her previous conclusions. Independently Soal carried out an experiment designed to test for telepathic abilities in the general public, and his statistical analysis of the results, published in 1932, showed once again absolutely no evidence for supernormal abilities.²⁸

It is against this background that the first experiments carried on at Duke University must be seen. The foundations for *Extra-Sensory Perception* were laid during the academic year 1930–1931, not by J. B. Rhine alone but also by other members of the university's new department of psychology (established

has rendered an immense service to this deprecated science. . . . If we accept the statistical laws of our time, there is no more to be said; Rhine has used them sensibly and under the guidance of experts. Where one might find fault with him is in the ease with which he accepts a fact as established by an average differing little from chance. It is a matter of degree; certainty is not established by a slight deviation from expectation. Decimals have never convinced anyone; doubt arises when the figures disagree with common sense." *Parapsychology*, trans. C. E. Green (New York: Grove Press, 1962), pp. 7–8.

²⁶Jephson, "Reply," p. 188. In this connection it is of interest to see Sudre's earlier remarks, "Psychical Research and Scientific Opinion," *Journal of the American Society of Psychical Research*, 1926, 20:335–342.

²⁷Jephson, "Reply," pp. 189–192.

²⁸*Theodore Besterman, S. G. Soal, and Ina Jephson, "Report of a Series of Experiments in Clairvoyance Conducted at a Distance under Approximately Fraud-Proof Conditions," *Proc. S.P.R.*, 1930–1931, 39:375–414; subsequently reprinted in Theodore Besterman, *Collected Papers on the Paranormal* (New York: Garrett Publications, 1968), pp. 292–328. Soal's studies on telepathy were published as "Experiments in Supernormal Perception at a Distance," *Proc. S.P.R.*, 1931–32, 40:165–362.

only four years earlier by the eminent psychologist William McDougall²⁹). Rhine himself began to carry out guessing contests in the summer of 1930, trying to find individuals who showed promise in guessing numbers or letters of the alphabet written down on cards hidden in the hand. In the fall of 1930 he was joined by Helge Lundholm in an attempt to study the possibility of telepathic communication in hypnotic trance, and by Karl Zener in further card-guessing experiments upon college students. It was Zener who at this time suggested introducing five simple designs as the targets for guessing, and these "Zener cards," as they were first called—the "E.S.P. cards" still in wide use today—became the basis for virtually all the experimental work done at Duke in the next few years. By the end of the spring semester of 1931 the first strikingly successful subject had been identified: A. J. Linzmayer, a Duke undergraduate, who in 600 guesses of Zener cards spread out over two months had correctly identified 238 cards instead of the 120 predicted by chance.³⁰

The advantages that the Zener cards held are obvious. Earlier researchers using playing cards had recognized that individual preferences for particular suits or ranks might distort chance expectations; but rather than devise a new experimental situation, they had chosen to try to appraise the weight of public preference attaching to different cards.³¹ Zener and Rhine had intended that the new designs would be more nearly neutral, would avoid playing cards' habitual associations; bearing pure designs, they were perhaps expected to carry the same force of imagery as the drawings that had been so popular among researchers at the turn of the century.³² More than this, however, the Zener cards made it possible to make experimental method conform to easy computation of results. *Extra-Sensory Perception* followed a statistical method far less cumbrous than that of Edgeworth, and one that allowed the researcher to measure degree of improbability, as the Fisher-Jephson approach did not. Rhine's practice was to determine the critical ratio (deviation from chance expectation/probable error) in each experiment or series of experiments, and to show how this might be converted into measured improbability. In the case of Linzmayer's first successes, he calculated the critical ratio as 17.9, and the consequent chance probability of these results as something of the order of 10^{-30} .³³

Between early 1931 and the middle of 1933, Rhine identified and studied seven other individuals who proved able to guess the Zener cards at a rate well above chance. All were Duke students, and (unlike Linzmayer) most had a marked interest in the field of psychology. With this increasing success tending

²⁹In associating Rhine with the Duke psychology department, McDougall was implementing what had been an interest in psychical research extending back many years and marked by his presidencies of both the British Society for Psychical Research (1920–1921) and the American Society for Psychical Research (1921–1922). For background on this interest of his, see J. B. Rhine, "The Importance of Parapsychology to William McDougall," *Journal of Parapsychology*, 1971, 35:168–188; for his writings on the subject, see Raymond van Over and Laura Oteri, ed., *William McDougall: Explorer of the Mind* (New York: Helix Press, 1967).

³⁰*ESP*, pp. 35–36.

³¹V. J. Wooley, "The Broadcasting Experiments in Mass Telepathy," *Proc. S.P.R.*, 1928–1929, 38:1–9; and cf. above, n. 23.

³²On the motivation behind the selection of the Zener cards, see *ESP*, pp. 50, 167, and J. B. Rhine, *New Frontiers of the Mind* (New York/Toronto: Farrar and Rinehart, 1937), p. 48.

³³*ESP*, p. 60.

to confirm the reality of the phenomena in question, the original extreme precautions taken to hide the card from the subject were slightly relaxed. Originally the experimenters Zener and Rhine had followed a procedure taken over from Ina Jephson of sealing the object cards in opaque envelopes. But—at first to canvass more possible subjects, and then to enable tests to be done more rapidly—Rhine (who had begun to carry on the experimentation alone) soon came to work in circumstances where the cards might be screened from the subject merely by a book or by the hand; occasionally the subject would hold the card in his hand, looking at its back, as he made his guess. The cards were not sealed away, either, for the D. T. (down through) experiments that Rhine began to carry on in 1932, though the precaution here must have seemed hardly necessary: in this situation the cards were shuffled, left in a stack, and the subject was asked to give the sequence of the cards in the stack without disturbing it. Similarly, for obvious reasons, the cards were not usually sealed away in the 1933 experiments carried on over considerable distances.³⁴ This casual attitude toward experimental precautions was widely criticized by reviewers of Rhine's early work, but in context it is certainly understandable, and it must be remembered that when particular care *was* felt necessary more cautious procedures might still be employed. It was only unfortunate that the Duke researchers did not always differentiate between results obtained informally and results obtained when unremittingly scrupulous precautions were employed, to guard against criticism of these consistently successful results.

III

The conclusions that Rhine eventually developed in *Extra-Sensory Perception*, his generalizations about the nature and occurrence of experimentally observed psychical phenomena, show (like his experimental technique) very strong resemblances to the conclusions of earlier researchers. Sometimes they were in fact based directly upon earlier work, which had suggested confirmatory experiments for the Duke laboratory to try out; at other times, it seems, Rhine's interpretation of his experimental data was guided by the views already present in the literature. His own sketch of the "Historical Background" to his work makes evident the extent to which his book built upon earlier foundations.³⁵ Yet despite this, there is a deeper, integrative level at which Rhine's work was indeed original. We can best appreciate this originality by considering individually some of Rhine's most important conclusions (neatly summed up

³⁴On Rhine's early methodology, see in particular *ESP*, pp. 50 ff. Gardner Murphy had written earlier in his article "Telepathy as an Experimental Problem": "An attempt to control all sources of error *at the beginning* is not only futile because of the impossibility of foreseeing all sources of error, but prejudicial to obtaining the kinds of occurrences one is out to observe. Tenseness, distrust, and apathy are but three of many ways of becoming negatively conditioned to a long series of laboratory experiments" (p. 275).

³⁵*ESP*, pp. 14–31. Rhine was already well acquainted with the literature of psychical research by 1929, before he had seriously begun his own experimental work with card-guessing, as the draft of a paper in the Rhine archives indicates (in the Manuscript Department, Perkins Library, Duke University). This paper, titled in pencil "Psychic Research—Seminar '29," gives a survey, with much historical background, of psychical research in general, telepathy and clairvoyance and their differentiation, trances, book tests, specified cross-correspondences, hauntings, poltergeists, and the spirit hypothesis.

in Part III of *Extra-Sensory Perception* under the heading “Explanation and Discussion”) in the light of their antecedents or anticipations in the earlier experimental literature.

The first elements of Rhine’s summary were based on his insistence upon distinguishing experimentally between clairvoyant perception (where the percipient was asked to identify a physical object) and telepathic perception (where the percipient was to pick out a mental image or impression). Conceptually, the distinction had long been made. It had been well recognized in the early days of the Society for Psychical Research that there were two possible different modes of “acquiring supernormal knowledge,” as Mrs. Henry Sidgwick put it in a paper of 1892.³⁶ In this paper she attempted to examine the evidence for “clairvoyance,” which she defined at the outset as “a faculty of acquiring supernormally, but not by reading the minds of people present, a knowledge of facts such as we normally acquire by the use of our senses,” thus explicitly excluding cases of “thought-transference,” or telepathy. However, while Mrs. Sidgwick was interested in the possibility of moving on from the telepathic phenomena she considered to have been established to a technique for demonstrating clairvoyance, she was not seriously concerned with the need for procedures that would separate the two types of psychical phenomena:

Cases like this [i.e., of thought-transference] show that though, if a faculty of independent clairvoyance exists, it may doubtless be exercised in the presence of persons cognisant of the facts clairvoyantly known, it can hardly under these circumstances prove its existence. But though the evidential reason for dividing off these cases from clairvoyance proper is clear, I am not prepared to say that the line so drawn has much scientific value. It is undeniable that such evidence as we have of clairvoyant perception of things at a distance is often very much mixed up with evidence of similar perceptions possibly due to thought-transference from persons present, and this suggests the possibility that clairvoyant perception of distant scenes is facilitated when it can be led up to by thought-transference from those present.³⁷

The English experimenters of the 1880s and 1890s were no more impressed than she, and made no consistent attempt to segregate the two classes of phenomena. Even though they might be conscious of the distinction, they seem not to have been alert to the possibility that one type of phenomenon might *interfere* with the experimental identification of the other.

Rhine was to develop the conceptual distinction between telepathy and clairvoyance into two distinct experimental situations which would rigidly separate instances of the one from the other. This experimental differentiation had its antecedent, too—in the work of Ina Jephson, as Rhine himself

³⁶Mrs. Henry Sidgwick, “On the Evidence for Clairvoyance,” *Proc. S.P.R.*, 1891–1892, 7:30. This marked something of an increase in objectification over the definition framed in the Society’s first year: “The distinction between thought-transference (mesmeric or otherwise) and clairvoyance we take to be that in thought-transference the idea or image is flashed into the recipient’s mind from some other mind in which it is already present, whereas the *clairvoyant* is said to discern at a distance inanimate objects, or persons whose thoughts are in no way directed towards him.” “Circular No. 1. On the General Work of the Society,” *Proc. S.P.R.*, 1882–1883, 1:300. In the following volume of the *Proceedings*, Gurney wrote of the “desperate improbability” of clairvoyance (*Proc. S.P.R.*, 1883–1884, 2:255, footnote).

³⁷Mrs. Sidgwick, “On the Evidence for Clairvoyance,” p. 31.

acknowledged.³⁸ In her 1928 paper Miss Jephson had described her own realization that one type of perception could mask the other—she suspected that clairvoyance might actually be the cause of apparent instances of telepathy—and had devised her experimental technique so as to exclude telepathy.³⁹ Rhine's early researches complemented experiments of this type with others designed to test for telepathy alone by asking the percipient to guess cards that were chosen mentally by the agent and noted down only after the guess had been made. It was in this systematic, self-conscious pursuit of discrete and well-defined isolable phenomena that Rhine was doing something new.

While Rhine was led to segregate instances of telepathy from instances of clairvoyance, he was nevertheless convinced by the evidence that the two were products of a common underlying process—and, moreover, that the so-called percipient was in fact the active entity in all psychical phenomena, whose conscious, voluntary effort was required to produce the effect. The feeling that thought transference or clairvoyance had to be consciously willed went back, again, far into the history of the S.P.R.; Oliver Lodge in 1889–1890 was commenting that “this kind of thought-transference without consciously active agency has never been experimentally proved. . . . Until it is actually established by experiment in the same way that conscious mind action has been established, it cannot be regarded as either safe or satisfactory.”⁴⁰ And indeed it is only in the last few years that any important evidence has been developed in favor of unconscious E.S.P.

But Rhine's emphasis upon the unique role of the percipient had not always been a commonplace. The S.P.R. had originated out of an attempt to investigate (or, for some of its members, to validate) the claims of spiritualism, which perhaps inevitably led them to consider thought transference as the most fundamental psychical phenomenon; they evolved the concept of agent and percipient as respectively the active and passive figures in telepathy,⁴¹ and,

³⁸ *ESP*, p. 27. Rhine commented, “I feel particularly indebted to Miss Jephson's work in that it helped to stimulate my own interest in clairvoyance.”

³⁹ Jephson, “Evidence for Clairvoyance,” pp. 227 ff. It should be remarked that Rudolf Tischner, in his book **Telepathy and Clairvoyance* (New York/London, 1925), p. 147, had already noted that “it is generally required, and justly so, of decisive experiments on clairvoyance (from which the possibility of telepathy is excluded) that nobody should know what the object under consideration is.” Tischner described his own experiments, mostly concentrated in the period 1918–1921, and argued that they provided convincing proof of the existence of pure clairvoyance. Tischner's differentiation between the two phenomena is found on p. 2 of this work.

⁴⁰ Oliver Lodge, “A Record of Observations of Certain Phenomena of Trance, (2) Part 1,” *Proc. S.P.R.*, 1889–1890, 6:453.

⁴¹ The assumption goes back to the origins of the Society: see, e.g., “Report of the Literary Committee,” *Proc. S.P.R.*, 1882–1883, 1:119 ff. It quite rapidly became an automatic and unconscious model for interpreting data—as, e.g., in Balfour Stewart's presidential address of 1887 (*Proc. S.P.R.*, 1886–1887, 4:265): “we find in these volumes [*Phantasms of the Living*] that an affection of the mind and body of A produces an affection of the mind and body of B by some unknown means, and often at a great distance. Now there are at least three conceivable hypotheses by which this action may be explained:—(1) the mind of A may act directly upon the mind, and through it upon the body, of B; or, (2) the mind of A may act directly upon the body, and through it upon the mind, of B; or, (3) the body of A may act in a peculiar manner upon the medium, and the medium may act upon the body, and through it upon the mind, of B. If the last hypothesis be correct, we may confidently hope to obtain something approaching numerical laws; but if the first hypothesis be true, it is more difficult to entertain this hope.” It is noteworthy that Balfour Stewart does not suggest the hypothesis that the mind (or body) of B might originate any activity.

as we have seen, gave second place to the problem of clairvoyance. In France, however, Richet maintained a much more critical attitude toward spiritualist explanations, and perhaps as a result treated telepathy as simply “a special and frequent case of cryptesthesia” (as he preferred to call clairvoyance).⁴² At least in part the English researchers came to share this view. In the late 1890s there are signs that telepathy was being construed as conceivably the positive act of a percipient,⁴³ and Gilbert Murray’s achievements twenty years later established the percipient’s importance for many. Jephson’s card-guessing experiments—and Rhine’s—are in a sense consequences of this shift in values which now made clairvoyance the type phenomenon for E.S.P.⁴⁴

The study of the presumed general mental function behind the two classes of phenomena led Rhine to enunciate a number of general rules applicable to the manifestation of E.S.P., bearing on the psychological and physiological state of the percipient. Once again, the individual generalizations were not novel discoveries, and Rhine himself repeatedly called attention to their enunciation in the earlier literature of psychical research. But they had rarely, if ever, been brought together as they were in *Extra-Sensory Perception* in a synthetic model defining conditions of experimentation for subsequent exploration.

⁴²Richet, *Thirty Years*, p. 604.

⁴³Cf. Richard Hodgson, “A Further Record of Observations of Certain Phenomena of Trance,” *Proc. S.P.R.*, 1898, 13:393. Support for the distinct possibility and even frequency of clairvoyance was also adduced at about this time by William Barrett in his study of dowsing: “On the So-Called Divining Rod,” *Proc. S.P.R.*, 1900–1901, 15:309 ff., 359 ff.

⁴⁴The French tradition of psychical research, exemplified by the work of Richet, tended both to stress the importance of clairvoyance over telepathy and to emphasize the active role of the percipient. Richet himself neatly juxtaposed these two elements: “It seems to me slightly less difficult to admit reading a signature at a distance than to admit reading a word in my brain; for, as we are dealing with the inexplicable, it is slightly easier to imagine that piercing vision should traverse the intervening distance and penetrate walls and papers than that the percipient should be able to interpret the verbal sense carried by the modes of vibrations of cerebral cells in my skull. Both being hypotheses, I prefer to imagine an amazing retinal vision of written words than a reading of my brain wherein nothing is written, but in which there are so many impressions, memories, and exceedingly complex and evanescent combinations that are really ultramicroscopic modifications of cellular protoplasms, none of which have any relation, apart from my own consciousness, to the sound or to the phonetic signs of a name.” *Thirty Years*, pp. 65–66.

The emphasis on the active role of the percipient had also an anti-spiritualist context, since it provided an alternative explanation—to that dependent upon discarnate spirits—for the way mediums and the like obtained their information. Significantly, Lodge took issue with just this passage from Richet’s book in defending the spiritualist hypothesis to which he himself adhered. Oliver Lodge, “A Text-Book of Metaphysics,” *Proc. S.P.R.*, 1924, 34:77 ff. Cf. also *Eugene Osty, *Supernormal Faculties in Man*, trans. Stanley De Brath (New York: E. P. Dutton, 1923), p. 161, and, esp. p. 134, where Osty emphasizes the activity of the percipient (in his cases, mediums) in an anti-spiritualist context, much as Rhine was to do. Cf. *ESP*, p. 129 (for the anti-spiritualist context of Rhine’s assertion that the percipient is active) and p. 145, n. 1, where Rhine gives Osty credit for emphasizing the active role of the percipient.

It is curious to find Hans Driesch maintaining in 1933 that clairvoyance (“if the supernormal acquisition of knowledge by the percipient relates to objective facts in nature”), telepathy (when “the agent gives his mental content actively . . . while the percipient ‘receives’ in a purely passive manner”), and thought reading (where “the percipient is active, he wants . . . to receive; the agent, who gives his knowledge, his mental content . . . , here plays a wholly passive part”) are three fundamentally different phenomena. *Hans Driesch, *Psychical Research* (London: G. Bell and Sons, 1933), pp. 68 ff. In admitting both telepathy and thought reading, Driesch appears to be occupying a transitional role between those early researchers who insisted upon the agent’s activity and the later ones, like Rhine, who emphasized the percipient’s role. Rhine (*ESP*, p. 147) commented upon Driesch’s decision to treat clairvoyance and telepathy as two fundamentally different phenomena; he did not mention that Driesch’s “telepathy” did not precisely coincide with his own.

By far the most heavily emphasized point in the book concerning the experimental context was the need for abstraction and attention on the part of the percipient, and the importance of techniques which would maintain these at a high level. Rhine cited Gilbert Murray's comments about his personal experience: "The least disturbance of our customary method, change of time or place, presence of strangers, controversy or especially noise is apt to make things go wrong."⁴⁵ Similar judgments go back to the very first decade of psychical research, even though the emphasis was more on the circumstances of the agent in telepathic experiments than on those of the percipient.⁴⁶ Schmoll and Mabire, for example, required of the agent "concentration of thoughts and looks on the object to be guessed, and firm determination that it shall impress itself upon the percipient's mental vision [the experiment was in what Rhine would have called G.E.S.P.], while the percipient, on his side, cannot sufficiently guard against all cerebral activity."⁴⁷ Then, as interpretation shifted in the period after 1900, the same conditions came to be recognized as essential to a percipient's success.

Linked to the emphasis upon maintaining the attention of the experimental subject was the desire to maintain his interest at a high level. The early work on card-guessing had served to convince many that experimental situations had to avoid monotony if the subject was to display continuing psychical ability. Mrs. Verrall offered this as one explanation why Murray was able to achieve his striking successes:

[His] experience is entirely consistent with observations in other telepathic experiments; the great difficulty is always to keep alive the interest of the percipient, and prevent the deadly boredom which comes from thinking of cards, or numbers, or diagrams, even in the case of persons whose scientific curiosity induces them to multiply experiments capable of "statistical" estimate as to success or failure. Such estimates are not to be undervalued, but we must deal with the telepathic faculty as we find it, and where the subjects selected for transmission are as varied, as complex and as distinctive, as they are in [Murray's work], there is little or no practical difficulty in estimating results, although they cannot be compared with what would be produced by chance as they can when selected numbers or playing cards are the chosen subjects and there is a calculable probability of success.⁴⁸

Rhine accepted this view completely. But, committed to card-guessing as he was, he had to develop other ways to keep the subject's mood light and relaxed—by joking with him, challenging him to better his score, varying the experimental situation in trivial ways, encouraging him, building up his confidence, never expressing doubt or hostility.⁴⁹ An easy, casual atmosphere became one hallmark of experiments at the Duke laboratory.

It was by presumed lapses of attention or of concentration, or attacks of

⁴⁵ ESP, p. 133, quoting *Gilbert Murray, "Presidential Address," *Proc. S.P.R.*, 1918, 29:58.

⁴⁶ Malcolm Guthrie, "An Account of Some Experiments in Thought Transference," *Proc. S.P.R.*, 1883–1884, 2:27. Cf. Lodge, as reported in *Phantasms of the Living*, Vol. I, pp. 50–51.

⁴⁷ *A. Schmoll and J. E. Mabire, "Experiments in Thought-Transference," *Proc. S.P.R.*, 1888–1889, 5:205. For similar judgments, see W. F. Barrett, "On Some Phenomena Associated with Abnormal Conditions of Mind," *Proc. S.P.R.*, 1882–1883, 1:238–244; Mrs. H. Sidgwick and Miss Alice Johnson, "Experiments in Thought-Transference," *Proc. S.P.R.*, 1892, 8:594–595; Guthrie, "An Account of Some Experiments," pp. 24–42.

⁴⁸ Mrs. Verrall, "Report on . . . 'Guessing,'" p. 84.

⁴⁹ The relaxed atmosphere of an extremely successful run carried out in 1932 or 1933 is described in Rhine's *New Frontiers of the Mind*, pp. 94–96.

boredom, that the early psychical researchers had been accustomed to explain a falling off of a subject's success over a long period of time, not merely in guessing cards, but in telepathic transmission of images.⁵⁰ It was Ina Jephson who first raised the possibility that these reflections of varying attention might be observable over short spans of time—for example, within the scope of a single run of card guesses.⁵¹ Her experiments with herself and others seemed initially to indicate a relatively higher proportion of successful clairvoyant guesses at the beginning of each run of guesses, and she attributed this “decline-curve” to the increasing fatigue of the percipient.⁵² Rhine was thus prepared for the discovery of both long-term decline of success and short-term curves of operation in his experiments. Even before he had begun to concentrate upon work with the Zener cards, in mid-1931, he had encouraged a graduate student in the Duke psychology department to look for the decline effect in guessing suits of playing cards⁵³ and had been informed of apparent success; subsequently Rhine identified the decline effect in the work of his principal subjects, Linzmayer and Stuart,⁵⁴ although he attempted to show experimentally that the decline was the result of weakening motivation and not of physical fatigue.⁵⁵ At the same time Rhine pointed out the likelihood of finding other curves,

“curves of operation”, better described perhaps as attention-curves. Just as we should expect interest, and consequently attention, to decline under some circumstances, so may we also expect greater attention to be given to some calls in the runs of 5 or 25 than to others. With most subjects the first call might be expected to be emphasized by greater effort and perhaps next in rank would come the last call of the run. It would be less likely if the middle call or another in the interior would be especially emphasized.⁵⁶

This notion, probably suggested by Rhine's reinterpretation in psychological terms of Jephson's results,⁵⁷ was confirmed by results of experiments with most of the Duke subjects, although certain individuals appeared to display characteristic and quite different curves of attention.

In the areas just discussed, it is clear that *Extra-Sensory Perception* adopted the attitudes of previous psychical researchers as to the proper psychological conditions of the experimental situation. There remains a final question which

⁵⁰See, e.g., Guthrie, “Further Report on Experiments,” p. 425; Sidgwick and Johnson, “Experiments in Thought-Transference,” p. 537; Edmund Gurney *et al.*, “Third Report on Thought-Transference,” *Proc. S.P.R.*, 1882–1883, 1:171. The latter reference is based upon the Society's experiences with the Creery sisters, but the fact that the Creerys were later detected in using a code of signals [Edmund Gurney, “Note Relating to Some of the Published Experiments in Thought-Transference,” *Proc. S.P.R.*, 1888–1889, 5:269–270] did not prevent the generalization, reinforced by cases where such collusion could be ruled out, from becoming accepted.

⁵¹Jephson, “Evidence for Clairvoyance” (1929), pp. 229 ff.

⁵²Miss Jephson called attention (*ibid.*, pp. 253–255) to the fact that G. H. Estabrooks' data revealed the same sort of curve, although Estabrooks had not attempted to offer any interpretation of it. Another suggestion of such a pattern had been made by Richet, “La suggestion mentale” (1884), pp. 626–627.

⁵³*ESP*, pp. 54–55.

⁵⁴*Ibid.*, pp. 135.

⁵⁵*Ibid.*, pp. 159–160. Miss Jephson had however already admitted that “the deterioration to which my experiments drew attention is due more to loss of spontaneity after the initial guess, than to fatigue” (“Reply,” p. 188).

⁵⁶*ESP*, p. 136.

⁵⁷*Ibid.*, pp. 141–142.

is natural for the experimenter to raise: is it possible and fruitful to inquire of the percipient what he thinks is taking place at the moment when he is achieving clairvoyant or telepathic perception? Psychical research had not adopted a consensus here. In the first few decades of experimentation, perhaps encouraged by the tendencies of the dominant school of psychology, Wundtian introspectionism, researchers had often tried to identify the nature of the process taking place within the subject's mind: whether the communication was mediated by words or by images, whether accompanying "thrills" were noted in successful instances, and so forth.⁵⁸ Increasingly, however, researchers in the 1920s came to mistrust the subject given to self-conscious examination or to an analytical interest in the experiment and his own thought processes.

The very worst type of man for an experiment of this kind is the instructor in psychology; second, only to him is the graduate student in the same subject. They simply cannot attack the problem in the proper spirit but insist on criticising the experiment and, much worse, reacting to it as they think it should be reacted to, and not as they are told to react. . . . In the future experiments the operator will also do considerable rejecting among his ordinary subjects. The moment a person becomes inquisitive, begins asking about types of imagery, criticises the experiment or appears over anxious or excited, the writer feels justified in telling him that his results will not be included.⁵⁹

It is tempting to see in this changing mood a reflection of the change in psychology, which by the 1920s had been deeply influenced by the behaviorism of John B. Watson. G. H. Estabrooks, whose remarks just quoted would seem to indicate a feeling that the experimental subject should be treated as little more than a mechanical selector of cards, had carried on the research he describes while a graduate student in psychology at Harvard. Rhine's reaction to this conflict of opinions was rather complex. Certainly he tried to find out how his subjects received their correct guesses, in terms of the feeling and imagery involved, but he was not remarkably successful and indeed arrived at the conclusion that the percipient "knows but cannot tell 'how he knows'; there is no analysis possible apparently."⁶⁰ In any case, he did not devote much attention to the problem, preferring to concentrate on other things: "The subjective exploration has not been pushed; first, because it is difficult to evaluate and, second, we must not, I think, start subjects to thinking too much about how they do it, lest we unfit them for successfully doing it by inducing self-consciousness, over-curiosity, or some other undesirable attitude."⁶¹ Certainly on the whole *Extra-Sensory Perception* showed much more serious and consistent interest in investigating the external, objective psychological and physiological conditions that accompany psychical phenomena than

⁵⁸*Oliver J. Lodge, "An Account of Some Experiments in Thought-Transference," *Proc. S.P.R.*, 1883-1884, 2:200. Introspectionism was most carefully and thoroughly applied to psychical research in Coover's 1917 monograph (n. 18); Coover's laboratory notebooks (now in the Duke University Library) reveal a model of such investigation.

⁵⁹Estabrooks, *Contribution*, p. 12.

⁶⁰*ESP*, p. 130.

⁶¹*Ibid.*, p. 142. When Pearce once scored 25 consecutive hits, in an obviously exceptional performance, Rhine reported that "the very strain he showed at the end was evidence of strong effort to concentrate attention. He said 'You'll never get me to do that again!' He could not describe his feeling further, however. (And, to avoid developing self-consciousness, I do not push introspective exploration.)" *Ibid.*, p. 134.

it did in exploring the internal, subjective content of the percipient mind. It was an attitude that would come to dominate future scientific parapsychology at Duke even more markedly.

IV

The mathematical and experimental techniques that were essential to J. B. Rhine's first research, and to his basic interpretative model, were (as we have seen) closely related to the psychical research of the previous half-century. So too were his attempts to appraise possible explanatory theories, to evaluate his phenomena and generalizations in the light of accepted scientific knowledge. These attempts, which are always interesting, well-informed, and sophisticated, were often developed out of the psychical researchers' debates.

This is clearest in the case of his consideration of E.S.P. in terms of the canons of contemporary physics. Rhine concluded that his experimental results were totally inexplicable by modern physical theory, since this would have to suppose some sort of radiation hypothesis involving the emission of waves both from the telepathic agent and from Zener cards, received selectively by the percipient. One of Rhine's arguments against such a theory arose out of the parapsychological experiments that he had had carried out over considerable distances. During 1933 he compared Hubert Pearce's card-guessing success at 2, 10, and 30 feet; then in 1934 at 100 and 250 yards. At the same time, two assistants in his research, Sara Ownbey and May Frances Turner, carried on an experiment in pure telepathy over 250 miles.⁶² On the assumption that radiant energy linked the cards or agent and percipient in such cases, success should have followed the inverse square law—that is, should have decreased with increasing distance—and yet in all cases the percipient performed at least as well at a distance as at close range. Rhine did not balk at drawing the radical conclusion—"bold" was his word—that "at this point we are then, it seems, faced with the need of another order of energy, not radiant," and presumably hitherto unknown to science.⁶³

While this may have been a radical conclusion, it was a response to a long-recognized difficulty.⁶⁴ The early investigators of thought transference, the leaders of the S.P.R., had included a considerable number of physical scientists, who had naturally tended to look into possible physical explanations for the phenomena they were observing. William Crookes, the discoverer of the cathode-ray tube and the element thallium, was (as indeed Rhine pointed out) the principal defender of the "radiation hypothesis." His presidential address to the S.P.R. in 1896, delivered in the excitement over Röntgen's discovery of X rays the previous year, included the suggestion that thought might be transmitted by similar radiation.⁶⁵ The idea of thought waves had certainly

⁶²On early distance experiments at Duke, see *ibid.*, pp. 83–86, 104–107. The earliest such was perhaps the series attempted by Harvey Frick in the summer of 1930 as background for his Duke M.A. thesis in psychology ("Extra-Sensory Cognition," May 1931, pp. 126 ff.); these experiments, while surely present to Rhine's mind, were not cited in *ESP*.

⁶³*ESP*, p. 122.

⁶⁴Cf. the discussion by Edmund Gurney, "Hypnotism and Telepathy," *Proc. S.P.R.*, 1888–1889, 5:227 ff.

⁶⁵William Crookes, "Address by the President," *Proc. S.P.R.*, 1896–1897, 12:338–355.

and not unnaturally been raised before, for in his own presidential address of 1894 A. J. Balfour (subsequently prime minister) had cited gravity and telepathy as analogous instances of "action at a distance" but had pointed out that in the latter case

. . . the whole character of the phenomena refuses to fit in with any of our accepted ideas as to the mode in which force may be exercised from one portion of space to another. Is this telepathic action an ordinary case of action from a center of disturbance? Is it equally diffused in all directions? Is it like the light of a candle or the light of the sun which radiates equally into space in every direction at the same time? If it is, it must obey the law—at least, we should expect it to obey the law—of all other forces which so act through a non-absorbing medium, and its effects must diminish inversely as the square of the distance. . . . [But] there is no evidence whatever that this equal diffusion actually takes place.⁶⁶

For the S.P.R. had of course amassed numerous accounts of spontaneous veridical phenomena witnessed hundreds and thousands of miles away from their source, and what experimental studies had been carried out along the same line certainly did not indicate a significant diminution of psychical effect with increasing distance. Crookes, referring to these points two years later, commented that "These are weighty objections, but not, I think, insurmountable."⁶⁷ Few other members of the Society, however, shared his confidence. F. W. H. Myers, in 1901, pointed out the further difficulties that would arise in trying to accommodate a wave theory to clairvoyance and to precognition,⁶⁸ and W. F. Barrett, in 1920, once again emphasized the problems posed for a radiation theory by its unremitting action over thousands of miles to a specific target rather than in every direction.⁶⁹ Thus Rhine was affirming the conclusions of earlier students of psychical phenomena, except that his identification of clairvoyance and telepathy led him to lay particular stress upon the difficulties of imagining how Zener cards could emit radiation that could be picked up by individual minds.⁷⁰

Rhine's suggestions as to the biological implications were somewhat more fully developed than his predecessors', and naturally enough, for Rhine's graduate training had been in biology, while earlier psychical research had

⁶⁶ A. J. Balfour, "Address by the President," *Proc. S.P.R.*, 1894, 10:10–11.

⁶⁷ Crookes, "Address," p. 352. He went on to say: "Far be it from me to say anything disrespectful of the law of inverse squares, but I have already endeavoured to show we are dealing with conditions removed from our material and limited conceptions of space, matter, form."

⁶⁸ *Frederic W. H. Myers, *Human Personality and Its Survival of Bodily Death*, 2 vols. (London: Longmans, Green, 1915), Vol. I, pp. 245–246.

⁶⁹ W. F. Barrett, "Note on Telepathy and Telergy," *Proc. S.P.R.*, 1920, 30:257–260. Barrett had begun as John Tyndall's laboratory assistant and had gone on to become professor of physics at the Royal College of Science, Dublin. Barrett's general opposition to "any physical analogy or materialistic explanation" of psychical phenomena was already clear in 1882. W. F. Barrett, "Appendix to Report on Thought-Reading," *Proc. S.P.R.*, 1882–1883, 1:62. Among the many other discussions of this issue, we might point to the detailed one of Gurney (*Proc. S.P.R.*, 1888–1889, 5:226 ff.), where he showed the difficulties in *either* assuming a vibratory energetic transmission (no falling off with distance) or not assuming a physical cause. He opted for a psychical force. It might be noted that possible evasions of this objection were still being raised by members of the S.P.R. in the 1930s: see the correspondence between G. W. Fisk and G. N. M. Tyrrell, *J.S.P.R.*, 1935–1936, 29:35–36, 41–42.

⁷⁰ For earlier echoes of this same difficulty, see Tischner, *Telepathy and Clairvoyance*, pp. 200 ff.

tended on the whole to attract more physicists than biologists (although Richet, Wallace, and Driesch were certainly eminent defenders). Biological experimentation and speculation had been correspondingly limited. It now seems odd, for example, that in the fifty years before the publication of *Extra-Sensory Perception* apparently only one experiment had been published which carefully assessed the variations of an individual's psychical powers when placed under the influence of drugs, although such experiments had certainly been proposed long before. Heymans, Weinberg, and Brugmans in 1919 had attempted to give telepathic commands to a young student, van Dam, as to which of a set of numbered checkerboard squares he was to point to. He enjoyed a significant degree of success, which was increased when he was given alcohol and decreased when he was given bromide. Yet the publication of this suggestive work apparently did not lead to similar tests elsewhere.⁷¹

Far more common than physiological experimentation had been speculation into the relationship between psychical abilities and mankind considered as a species. Was it the evolutionary product of natural selection? Was it present in all mankind or only in a few isolated individuals? One might have expected Alfred Russel Wallace to have taken the lead in such speculations, but he tended rather to the belief that the evolutionary process could in no way explain psychical phenomena.⁷² Myers was actually among the first to try seriously to interpret telepathy in the light of natural selection, when in 1886 he suggested that it might be akin to "certain forms of sensibility which we perceive, in some rare examples, to be possible to the human organism, but which have not, apparently, been valuable enough to our ancestors to get themselves established among recognized human faculties."⁷³ Telepathy was in this view a feature of biological life which had never been fully developed. Myers elaborated on these ideas in 1892:

These faculties, I say, cannot have been acquired by natural selection, for the preservation of the race, during the process of terrene evolution. In some sense they pre-existed; they were (as we may phrase it) the products of pre-terrene evolution. . . . In [man] and in his ancestors were many faculties which were called out by the struggle for existence, and became supraliminal. But there were many faculties also which were not thus called out, and which consequently remained subliminal. . . .⁷⁴

⁷¹ See above, n. 20. Speaking of Brugmans' use of drugs, Gardner Murphy wrote: "Two familiar ideas from the psychology laboratory were introduced: one, the effects of drugs, the other, the effect of relaxation." "Telepathy as an Experimental Science," in Carl Murchison, ed., *The Case For and Against Psychical Belief* (Worcester, Mass.: Clark University, 1927), p. 270. Some other researchers did think of studying the effect of drugs upon telepathic communication but did not publish any detailed accounts of their work. E.g., Louisa Rhine, who with her husband was abstracting foreign periodicals for the *Journal of the American Society for Psychical Research* in 1925–1926, called attention to some tentative French experiments with peyote in *J.A.S.P.R.*, 1926, 20:124–125.

⁷² Wilma George, *Biologist Philosopher. A Study of the Life and Writings of Alfred Russel Wallace* (London/Toronto/New York: Abelard-Schuman, 1964), pp. 243 ff; Malcolm Jay Kottler, "Alfred Russel Wallace, the Origin of Man, and Spiritualism," *Isis*, 1974, 65:145–192.

⁷³ Frederic W. H. Myers, "Automatic Writing," *Proc. S.P.R.*, 1887, 4:259. Some remarks made the previous year suggest a more positive attitude: "Telepathy is surely a step in *evolution*. To learn the thoughts of other minds without the mediation of the special senses manifestly indicates the possibility of a vast extension of psychical powers." "Automatic Writing," *Proc. S.P.R.*, 1885, 3:31–32.

⁷⁴ Frederic W. H. Myers, "The Subliminal Consciousness. Chapter III," *Proc. S.P.R.*, 1892, 8:359. The phrase "pre-terrene evolution" is revelatory of Myers' commitment to spiritualism. See Alan Gauld, *The Founders of Psychical Research* (London: Routledge and Kegan Paul, 1968), esp. Ch. 4.

And Myers seems to have felt that telepathy and clairvoyance were of this sort. Variations on this idea were subsequently taken up:

Is it not in itself likely that here and there we should come across rudimentary beginnings of such senses [i.e., psychical powers]; beginnings never developed and probably never to be developed by the operation of selection; mere by-products of the great evolutionary machine, never destined to be turned to any useful account? And it may be . . . that in these cases of the individuals thus abnormally endowed, we really have come across faculties which, had it been worth Nature's while, had they been of any value or purpose in the struggle for existence, might have been normally developed, and thus become the common possession of the whole human race.⁷⁵

As the quotation just given shows, evolutionary interpretations of this sort could help explain the depressing fact that psychic power was neither widely nor easily demonstrable in man.

Rhine's book pursued both these lines of investigation, through physiology and through evolution. He developed Brugmans' work, for example, by administering both sodium amytal and caffeine to his subjects and determined that their rate of scoring fell off and increased under the influence of the respective drugs; this he took to be consistent with the Dutch work as indicating a need for "a certain degree of integration of the nervous system."⁷⁶ Taken in conjunction with the apparent need for alertness in subjects, these results led him tentatively to separate experimental successes from the spontaneous occurrences that often took place in dissociated conditions, such as drowsiness or sleep. Given the fact that sensory phenomena remained stable when, under administration of amytal, extra-sensory perception disappeared, Rhine also advanced the suggestion that "E.S.P. may be a higher, more complex development than sensory perception and, the suggestion follows logically, is probably a later development in mental and cerebral evolution. This is unfavorable to the view held by some without even this amount of facts to support it, that E.S.P., in man, is an atavism."⁷⁷ The consistent if slight success that Rhine claimed to have had with virtually all the seventy-seven students the Duke laboratory had tested—and the quite marked ability that extensive testing had revealed in several of the department's graduate students—led him to suspect further that "for aught that may be said to the contrary, E.S.P. may be as widely distributed a natural capacity in the species as is the highest mode of cognition, reasoning."⁷⁸ The fact that his major subjects all averaged six to ten successes in twenty-five guesses suggested a "species level" corresponding to "native species endowment," though individuals could certainly transcend this level on occasion. Rhine's unprecedented experimental success had thus led him in this one respect to break away from earlier views.

Extra-Sensory Perception is more closely tied to psychology than to either physics or biology, by virtue of Rhine's being a member of a psychology department. Perhaps inevitably, therefore, his attempts to make psychological sense of his experiments were deeper than his remarks on the other sciences, and this increased thoughtfulness meant that his treatment would suggest lines of

⁷⁵ Balfour, "Address" (1894), p. 7.

⁷⁶ *ESP*, p. 127.

⁷⁷ *Ibid.*, p. 156.

⁷⁸ *Ibid.*

research for future parapsychologists. Earlier workers had certainly shared his view that psychical phenomena were closely akin to psychological phenomena. Rhine himself made this clear in selecting the term “parapsychology” to denote the field: “The German usage of ‘parapsychology’ for the general field seems a little more generally appropriate than the others, if we do not use the prefix as implying that psychical research is outside the field of psychology—but simply that it is ‘beside’ psychology in the older and narrower conception.”⁷⁹ But in the previous fifty years no serious attempt had been made to integrate the study of psychical phenomena into the domain of psychology *per se*. Most of the earlier investigators were not in any remote sense professional “psychologists,” even if they might have developed a great interest in and knowledge of psychology, as Myers had. Those interested in psychical research who clearly were psychologists—like William James and William McDougall—had little personal concern for the experimental aspect of the subject.⁸⁰ Rhine’s American academic predecessors, Coover, Troland, Murphy, and Estabrooks, carried on experimental work fitfully at best. None of these men committed himself to a sustained program of experimental study, as Rhine was to do.

Rhine’s commitment must be understood in the context of his professional situation—as a member of a university psychology department devoting almost all his research to experimentation on telepathy and clairvoyance while teaching courses on psychology. In these circumstances, Rhine’s main interest was naturally in the psychological aspects of E.S.P. and focused on the situation of the psychological experiment: the conditions of stress and calm, attention and relaxation, novelty and boredom, which seemed to affect the manifestation of telepathy and clairvoyance. As has already been shown, such concerns were not new with Rhine, and comments on each of these factors can be found in the literature before Rhine. But he gave them order and at least the outline of a psychological framework by attempting to relate psychical phenomena to other known and testable mental features: perception, cognition, motivation, personality, and learning. Thus, in considering the so-called decline-effect over time in the ability to achieve correct guesses, Rhine wrote:

We have had decline-curves of several kinds, in the run, in the series, for a season, and for years—but no ascending-curves. We have been interested in learning and development curves too, but E.S.P. does not appear to be a developmental matter, as our data reveal it. . . . Rather does it seem that we have here a basic function that can easily be inhibited but not developed. It is probably as innately given as is sensory perceptual capacity. This is, however, a point for further inquiry!⁸¹

In addition, Rhine regularly preceded his accounts of his subjects’ performances with personality profiles, and in his general summary, “The Psychological Conditions,” he made some tentative essays at correlating personality traits and E.S.P. abilities.

As to personality traits and E.S.P., there are some suggestions or general impressions, as yet undeveloped; for example, possible correlations between E.S.P. and a tendency

⁷⁹ *Ibid.*, p. 7.

⁸⁰ The latter’s writings on psychical research may be studied in *William McDougall: Explorer of the Mind* (see above, n. 29); the former’s, in *William James on Psychical Research*, ed. Gardner Murphy and Robert O. Ballou (London: Chatto and Windus, 1961).

⁸¹ *ESP*, p. 142.

to daydreaming and high imaginativeness. Stuart suggested a correlation between E.S.P. and artistic interest and ability that seems promising. Hypnotizability, too, is about on a par with these, in correlation with E.S.P. Sociability has a chance, too, of being correlated. But our data are yet too limited on this line of comparison. Relations of E.S.P. to age, race, and other stages and conditions, too, are yet for the future.⁸²

The last phrases of both these passages give a clue to the role of the psychological framework in the book *Extra-Sensory Perception*: it is not really central to the material being reported, but rather is used as a guide to the development of future research—by which parapsychology would be more closely relatable to experimental psychology.⁸³ Many of these research suggestions were indeed followed up (and are still being pursued) by parapsychologists working with Rhine and elsewhere.

If Rhine went beyond his predecessors in trying to develop links with experimental psychology, he did not share the interest in most speculative areas of psychology that others had manifested; in particular, he gave short shrift to neurological and psychodynamical speculations. Rhine did admit that E.S.P. “depends upon the higher functions of the nervous system,” but beyond this he did not go. He gave even less attention—really none at all—to a possible psychodynamical model that would relate E.S.P. to unconscious mental processes.

In both these areas of speculation, Rhine's attitude differed from that of some of the earlier psychological researchers, in particular F. W. H. Myers. An avid follower of the latest developments in psychology during the 1880s and 1890s, Myers had been interested in both neurological and psychodynamical speculations: the former in connection with the attempt to devise a localization-of-brain-function model to explain “automatic writing”; the latter in connection with his development of a generalized conception of the unconscious (the “subliminal self,” he called it) to account for the host of strange mental states which were intriguing psychologists and psychiatrists in the last decades of the nineteenth century—hypnosis, hysteria, multiple personalities, and psychical phenomena.⁸⁴ Myers was in close touch with the French investigators, especially Janet, and he was one of the first to publish an appreciative account of Freud and Breuer's work on hysteria, in 1893.⁸⁵ With Rhine, grandiose speculations like those of Myers—fascinating, but not readily testable—were put aside in favor of a program to demonstrate experimentally and statistically the existence of psychical phenomena and their variation with change in conditions and over time.

V

Extra-Sensory Perception appeared in print in April 1934, and a second American printing was made within a year. It was first printed in England in June

⁸² *Ibid.*, p. 143.

⁸³ A still more overt expression of the psychological implications which Rhine recognized in his work may be found in his article “Extra-Sensory Perception of the Clairvoyant Type,” *Journal of Abnormal Social Psychology*, 1934–1935, 29:151–171, which introduced his research to the psychological community.

⁸⁴ Gauld, *Founders*, pp. 275–299.

⁸⁵ Frederic W. H. Myers, “The Subliminal Consciousness,” *Proc. S.P.R.*, 1893–1894, 9: pp. 7, 12 ff. Ernest Jones implies that this was the first such account. *The Life and Works of Sigmund Freud*, 2 vols. (New York: Basic Books, 1953), Vol. I, p. 250.

1935 (by Faber and Faber, in a slightly revised version), but it had already come to the attention of the Society for Psychical Research, since a copy of the April printing had been sent them for review, and others were certainly available.⁸⁶ By December 1934 it had “aroused . . . much interest” in the Society.⁸⁷ The book was given to Robert H. Thouless to review, on the face of it an odd choice, since such figures as S. G. Soal and Ina Jephson were seemingly better placed to evaluate Rhine’s work.⁸⁸ Thouless was forty and had been in charge of the department of psychology at Glasgow for eight years. His research had lain entirely in “orthodox” subjects—social psychology, psychology of religion, hypnosis—and his only contact with the field of psychical research had been his participation in an investigation of the claims of psychical ability made for the hypnotist Wallenius.⁸⁹ He was himself not convinced of the reality of psychical phenomena and was apparently asked to review Rhine’s work simply because of his professional status, as a psychologist appraising a colleague’s work. His review was read at a meeting of the Society on January 30, 1935, and published immediately in the *Proceedings*.⁹⁰

Broadly speaking, however, Thouless’ reaction to *Extra-Sensory Perception* must have been that of the leaders of the S.P.R. He failed entirely to comment upon the theoretical or speculative elements in the book, and we can easily see why these features would not have excited comment from an audience of English psychical researchers, for the book’s conclusions are of course in no significant respect different from those laboriously hammered out by the English tradition; indeed, they are often built upon them. The distinction between telepathy and clairvoyance, on which Rhine placed considerable emphasis, was mentioned by Thouless, but in the context of the Duke techniques for *demonstrating* the distinction, the conceptual distinction being well established by this time in England.⁹¹ It was in fact Rhine’s experimental method that consistently captured Thouless’ attention. This was not merely a matter of the introduction of the Zener cards, although Thouless certainly did emphasize their use in place of playing cards as “one of the most important changes that Rhine makes in method,” commenting that they were much more easily

⁸⁶ Harry Price received a copy of the book from America on May 11, 1934, and immediately called S. G. Soal’s attention to it; his correspondence with Soal indicates that by July 1934, six months before Thouless presented his review, Soal was arranging to have “Rhine cards” printed in order to try Rhine’s technique for himself (Harry Price Collection, University of London).

⁸⁷ *J.S.P.R.*, 1934, 28:315.

⁸⁸ Initially Theodore Besterman—then librarian, investigations officer, and editor of the *Journal*—expected to review the book, but when pressure of work forced him to give up the editorship, the review was assigned to Miss Jephson (Besterman to Rhine, June 14, 1934; Duke University Library). Why and when responsibility for the review passed from her to Thouless is not clear.

⁸⁹ F. H. G. van Loon and R. H. Thouless, “Report of a Demonstration of Experiments by Mr. Gustav Wallenius . . .,” *Proc. S.P.R.*, 1928, 36:437–454.

⁹⁰ Robert H. Thouless, “Dr. Rhine’s Recent Experiments on Telepathy and Clairvoyance and a Reconsideration of J. E. Coover’s Conclusions on Telepathy,” *Proc. S.P.R.*, 1935, 43:24–37.

⁹¹ In the annual report of the council for 1934, read at the meeting of the Society of Jan. 9, 1935, the following was reported: “Several of our members have been conducting experiments in paragnosis [psychical abilities] of various kinds. The work of Dr. J. B. Rhine, now the Honorary Research Officer of the Boston Society for Psychic Research and a member of our Society, on extra-sensory perception is of great interest. In it he has emphasized the importance of conducting experiments in such a way that results attributable to any one particular form of paragnosis shall be distinguishable from results attributable to other forms, and this is being borne in mind by the experimenters of our Society.” “Annual Report of the Council for 1934,” *J.S.P.R.*, 1935–1936, 29:23–24.

"imaged."⁹² Nor was it an interest in other specific aspects of Rhine's experimental procedure, such as his preselection of gifted subjects for extensive testing. In any case, Rhine's method was in structure very closely related to earlier English research. The real importance, the real originality of Rhine's work for Thouless was that it had apparently been so consistently and continuously *successful*.

Successes on the scale reported by Rhine were totally unprecedented in psychical research in 1934. The S.P.R. had witnessed innumerable disappointments over fifty years in experimental studies. Tests that had initially seemed quite promising were soon called into question or had shortly yielded only chance results, from the case of the Creery sisters in the 1880s to Miss Jephson's work of the late 1920s. The increasingly strong emphasis upon investigations of mediumship within the Society was simply the converse of its growing conviction that psychical powers, if they existed, could only be the gift of a vanishingly small number of individuals, or would only be demonstrable under irreplicable circumstances, or perhaps both. Now Rhine's work seemed to suggest that his simple techniques could be used to reveal "measurable telepathic and clairvoyant capacity in as many as one in three or four persons."⁹³ Precisely because of the striking novelty of this feature of the Duke work, Thouless took Rhine to task for his apparent lapses in experimental rigor and his failure to fully report the experimental conditions:

Dr Rhine's procedure is by no means free from objection, and his presentation is open to the much graver objection that the experimental methods are quite inadequately reported. This is a pity, since a little more care in reporting . . . would have made this work very much more convincing. It may be that all that Dr Rhine reports is true, but much of his report will not carry much conviction to those inclined to be sceptical. At least we may say that Dr Rhine has shifted the burden of proof on to those who deny that extra-sensory perception is a fairly common capacity.⁹⁴

Rhine felt that Thouless' comments on the frequency of psychical abilities had really missed his point, as appears from a response to the review that appeared in the *Proceedings* late in 1935:

Professor Thouless states: "The novelty of Dr Rhine's results lies in his apparent demonstration that this power is not uncommon and it is here that his evidence is quite inadequately stated." I call especial attention to the fact that this emphasis on frequency is the reviewer's point of view, not that of the book reviewed. . . . We were working for bigger stakes than frequency of distribution of E.S.P. ability. Our objective was to try to explain it as far as possible.

In the judgment of most critics the "novelty" of the Duke work lies in its experimental separation for the first time of telepathy and clairvoyance, and of

⁹²Thouless, "Dr. Rhine's Recent Experiments," p. 31. Thouless expressed this feeling still more strongly in a letter to Rhine: "I should think it very likely that the reason for your much higher percentage of successes than those reported by other people is that the Zener cards are more satisfactory material for transmission than the ordinary playing cards" (Thouless to Rhine, Jan. 24, 1935; Duke University Library).

⁹³*Ibid.*, p. 25.

⁹⁴*Ibid.*, p. 37.

their independent demonstration. It is much more novel than the number of subjects found.⁹⁵

Yet perhaps this reply of Rhine's in turn missed Thouless' point, for it certainly fails to reveal any appreciation of what would have struck the psychical-research tradition as the outstandingly significant feature of his experiments. His experimental differentiation of telepathy and clairvoyance was no doubt important, but to the S.P.R. it was still an articulation and substantiation of previous work. What explanations of E.S.P. ability he offered were not fundamentally novel. But his claim that, for all that might be said to the contrary, E.S.P. ability might be as widely distributed as the power to reason was something that nothing in half a century of study had prepared the S.P.R. for. In particular, the negative results published by Soal in 1931 and 1932 (to which Rhine had given no serious consideration) had convinced many Society members that telepathy and clairvoyance were not general powers of mankind. Although Rhine cited Estabrooks and Coover as earlier figures whose research had shown that psychical abilities were not difficult to find, Estabrooks' paper had not made it at all clear whether his positive results were the contributions of a few or many of his individual subjects,⁹⁶ and Coover's work had until Rhine's own analysis in *Extra-Sensory Perception* been generally taken to have produced no positive results at all. Indeed, Thouless devoted nearly half his review of Rhine's book to a résumé and elaboration of Rhine's analysis of Coover's work, "since it is a common opinion that Coover's results were entirely negative and show nothing but chance distribution."⁹⁷ The revelation that the piece of experimental research which had seemed to be the single most damaging attack on psychical abilities actually contained evidence, previously unrecognized, for such powers was in its own way just as encouraging as Rhine's own experimental results. *Extra-Sensory Perception*, then, was noteworthy for its promise that simple experimental techniques could be devised that would demonstrate psychical ability in a large percentage of unselected subjects.⁹⁸

It is not of consequence to this study to follow in detail the reaction of psychical researchers to Rhine's book, but it is certainly not irrelevant to make it clear that his work (which was of course pursued unremittingly at Duke University for nearly three decades) did give a new impetus to the experimental study of psychical phenomena in England as well as in this country. The text of Thouless' review in the S.P.R. *Proceedings* was followed directly by an appeal

⁹⁵J. B. Rhine, "Note on Professor Thouless' Review of *Extra-Sensory Perception*," *Proc. S.P.R.*, 1935, 43:542-544.

⁹⁶"The experiments described in this paper are directed to the question whether a trace of that power is a common possession, so that when the results from many ordinary persons are aggregated that trace may be perceptible" (Estabrooks, *Contribution*, p. 27, n. 1).

⁹⁷Thouless, "Dr. Rhine's Recent Experiments," p. 25. In fact, F. C. S. Schiller's review of Coover's book had also argued that there was "a source of rightness beyond chance" in the experiments that Coover recorded (*Proc. S.P.R.*, 1920, 30:261-273). The point was repeated by Richet in *Thirty Years*, pp. 93-94.

⁹⁸There is certainly evidence to suggest that committed professional psychical researchers felt this as well as Thouless. Harry Price described "the greatest phenomena revealed by [*Extra-Sensory Perception*]" in these words: "whereas for many years psychists have been scouring the world for first-class percipients, and failing to find them, Dr. Rhine discovered a batch of them in his own university—even among his own students. I think this is truly remarkable." (Letter to the editor of *The Listener*, July 25, 1934; Harry Price Collection, University of London.)

from the secretary of the Society, W. H. Salter, speaking for its council, and asking for members' participation in "further experiments (a) with assessable material, (b) under strict conditions, and (c) in such a form as to distinguish between the different types of extra-sensory perceptions."⁹⁹ It is not merely Salter's unforced use of that last phrase that reveals the influence of Rhine's book; his note makes it clear that it was "the high percentage of success obtained by Dr. Rhine" that had led the council to pursue the problem formally. The individual singled out to direct the new experiments was G. N. M. Tyrrell, who had been inspired by Rhine's book a few months before to renew his own researches.

In 1921 Tyrrell had carried out several series of varied experiments with a young girl, Gertrude Johnson, who had repeatedly succeeded in guessing the denominations of the first half-dozen in a freshly shuffled deck of cards.¹⁰⁰ He "had no opportunity to experiment at length" with Miss Johnson until 1934, when Rhine's book stimulated him to return to experimental psychical research. During the fall of that year Tyrrell devised a special apparatus for demonstrating generalized extra-sensory perception which did not involve card-guessing, although Rhine's methods inspired certain features of its form. Working once more with Miss Johnson, Tyrrell again obtained some success, reported in the spring of 1935,¹⁰¹ and it was because of this independent interest that Tyrrell was chosen by the council to conduct the new series of experiments. Other members of the Society attempted on their own to repeat Rhine's success by using the Zener cards—Soal (who had taken part in the unsuccessful attempt of 1929–1931 to duplicate Miss Jephson's first results) being the most important of these.¹⁰² But here we need not go into an elaborate account of any of this work. It is certainly clear that the publication of *Extra-Sensory Perception* was responsible for the revival of interest in England in carrying on large-scale, rigorous, quantifiable experimentation into psychical phenomena.

Its publication caused less stir on the Continent, no doubt because the experimental approach to the investigation of telepathy and clairvoyance was far less well established there. But it was still not viewed as a markedly new approach. The book was reviewed in the *Revue Métapsychique* by César de Vesme, who emphasized its resemblance to earlier French research: "Professor Rhine follows these 'principal conclusions' with a listing of a series of other points of lesser importance which, besides, are generally conformable to what earlier experimenters have noted. . . . The difference between the 'Extra-Sensory Perception' of Dr. Rhine and the 'Connaissance Paranormale' of Dr. Osty is very little."¹⁰³ The most important German review was that by Hans

⁹⁹W. H. S[alter], "An Appeal for Co-operation in Further Experiments in Extra-Sensory Perception," *Proc. S.P.R.*, 1935, 43:38–39.

¹⁰⁰Mr. T—, "The Case of Miss Nancy Sinclair," *J.S.P.R.*, 1920–1922, 20:294–327. (Sinclair was a pseudonym of Johnson's.)

¹⁰¹G. N. M. Tyrrell, "Some Experiments in Undifferentiated Extra-Sensory Perception," *J.S.P.R.*, 1935–1936, 29:52–71.

¹⁰²Soal learned of the book's existence in mid-May and by July had sketched out a procedure for large-scale testing of Rhine's results. It took several months to get Zener cards printed up carefully enough to exclude imperfections, but the experiments were launched in December. The progress of Soal's work during this period can be traced in his correspondence with Harry Price (University of London).

¹⁰³*Revue Métapsychique*, 1934, No. 4:262–265.

Bender, in the *Zeitschrift für Psychologie*. Bender had begun his own laboratory experiments on clairvoyance in the psychology laboratory at Bonn in 1932, knowing nothing of course of Rhine's work, and reacted to *Extra-Sensory Perception* with delight, describing it as "a work which, despite the heretofore and still controversial character of its theoretical conclusions, emphasized by the author, is still capable of freeing future works in this area from proving the justifiability of such investigation and of leading them directly into the numerous problems that it raises for inquiry."¹⁰⁴

In America, *Extra-Sensory Perception* met an enthusiastic response from a young psychologist who had himself been deeply if intermittently engaged in the experimental side of psychical research for many years—longer, indeed, than Rhine himself—Gardner Murphy, at Columbia. Murphy had assisted Troland in the latter's experiments at Harvard in 1916–1917, had himself been the recipient of the psychical research fund there from 1922 to 1925, and had subsequently maintained his interest in the field while establishing himself in mainstream psychology. To Murphy, as to Bender, Rhine's book marked a watershed in psychical research: at last, it seemed, a technique had been devised for getting telepathy and clairvoyance under experimental control.¹⁰⁵

VI

We began by raising the question: in what sense can *Extra-Sensory Perception* be said to have been a focal, paradigmatic work for parapsychology? We have just seen that it immediately stimulated renewed interest in experimental research along its own lines because it reported an unprecedented degree of success. In this connection we should reemphasize that the preexisting tradition of psychical research was bound to take Rhine's work with particular seriousness because it conformed so well to virtually everything that that tradition felt it had learned. The book brought together the conclusions of a great number of different researchers of the past who had worked in a remarkably wide range of fields—dowsing, mediumship, experimentation of all sorts, parlor games—and it presented these conclusions, found piecemeal in the earlier literature, as all derivable from a single model experimental situation, a situation which itself had strong elements of familiarity. In this sense, Rhine's book resembles other paradigmatic works which give orientation and focus to a field, such as Benjamin Franklin's letters of the 1740s on electricity, to give an often-cited example.

In the United States the paradigmatic character of Rhine's work was reinforced by the institutional support given parapsychology at Duke. The university provided Rhine with the opportunity, then unique, to continue his investigations

¹⁰⁴ *Zeitschrift für Psychologie*, 1935, 135:218–219.

¹⁰⁵ "In comparing Dr. Rhine's book with all previous experimental studies, I should venture the statement that what counts in science is what is witnessed in such amount and under such conditions that its laws become decipherable, and that the present volume is the first sustained attempt to gather the material under defined conditions with true comprehension of the nature of the difficulties and the ways of surmounting them." G. Murphy, *Journal of General Psychology*, 1934, 11:458. For details on Murphy's career in psychical research, see his own account: "Notes for a Parapsychological Autobiography," *J. Parapsy.*, 1957, 21:165–178.

in his own laboratory with assistants drawn from the school's doctoral program in psychology. By 1937 enough work had been undertaken to warrant the establishment of a new journal, the *Journal of Parapsychology*, whose first volume included accounts of experiments carried on with a wide variety of perceived objects (tests using the normal symbols were compared with others using distorted symbols or even colors) and subjects (the blind, twins, retarded, and normal children).

In certain respects, however, Rhine's work fits the customary paradigmatic model less well, for reasons which have to do with the inherent difficulties of experimentation in parapsychology. While Rhine's work certainly did cause experimental investigations in the field to focus upon the sort of situation he had devised, his claims of success were not immediately echoed by all other psychical researchers. In England, where the most elaborate attempts at independent replication of his work occurred, there had been some skepticism of his claims and criticism of his methods when *Extra-Sensory Perception* first appeared. This criticism might have quickly passed away, to be remembered merely as another case of resistance to a major scientific reorientation, had there not been considerable difficulty in repeating his achievements. During the 1930s, only Tyrrell claimed to have done so, and many of his colleagues in the S.P.R. expressed dissatisfaction with *his* methodology as well. Although subsequent investigations have served to convince most parapsychologists of the validity of Rhine's general claims, the problem of experimental replication continues to set this field off from most other sciences. It remains true, nevertheless, that Rhine's experimental aims and methods became widespread in the 1930s even among those psychical researchers who could not accept his attribution of E.S.P. to mankind in general.

Extra-Sensory Perception also is different from most works that have become the foundation of a new science in that it offers no explanatory theory for the phenomena it describes. Rhine presented neither a physical mechanism nor a psychodynamical model or anything else to make sense of telepathy and clairvoyance. In drawing together the ideas of the previous half-century of psychical research, he presented elements of what could conceivably become such a theory, as when he laid stress upon the activity of the percipient rather than the agent, and presented new correlations that would need to be accounted for by any unifying explanation, but he himself was not prepared to articulate a developed theory. As it happens, it remains one of the frustrations of parapsychologists today, forty years later, that they can devise no satisfactory unifying theory for their data. Nevertheless, this has not affected their awareness of their own identity. Their self-awareness depends not on a shared theory but on shared language, methods, and problems, and these ultimately can be traced back to *Extra-Sensory Perception*. Despite all the qualifications made necessary by the peculiar nature of this strange field, we feel that it is in fact fruitful to consider *Extra-Sensory Perception* as something of a paradigmatic work for parapsychology in that it synthesized previous thought and work and set a pattern for subsequent experimental research.¹⁰⁶

¹⁰⁶ For a philosopher's treatment of this same general topic, see Ingemar Nilsson, "The Paradigm of The Rhinean School. Part I," *European Journal of Parapsychology*, 1975, 1:45-59. Part II, which will appear shortly, we have seen in draft.